contemplated when the grant was originally made. One would think that such provision should be made by special Parliamentary vote. Further, on the same page, it will be seen that the sum of 80,000*l.* is allocated to five institutions which did not receive a penny from this source in the year 1920–21. Two of them—Oxford and Cambridge—are each to receive 30,000*l.* Now we do not for a moment begrudge them these grants. But, by extending the field of the distribution, a large sum, in the cases just mentioned 106,030*l.*, has been diverted from institutions which otherwise would have benefited from it, and this fact ought not to be overlooked.

It cannot be too strongly urged that Universities and institutions of University rank are in an anomalous position in that they are compelled by force of circumstances to look to the Government for assistance. Their financial burdens, largely due to the crisis through which the country is passing, cannot be met from their normal sources of income. Benefactions are problematic. To raise the fees to meet the additional and necessary costs would be to make them so high as to prevent a large number of deserving students from entering the University, with ultimate loss to the community and nation. Already the fees charged are considerably larger than those which prevail in the United States of America. It is facts such as these which make the problem of University finance so difficult and the necessity of further Government assistance so imperative.

If our legislators have any doubt about this necessity, let them examine the figures on p. 54 of the Estimates, and note the relative disparity between the grants for England and Scotland. Six Scottish institutions are to receive 180,000*l.*, whereas forty-two English institutions will get only 591,180*l.*! A footnote makes it clear that the Scottish estimate includes 72,000*l.* awarded by Scottish Acts of Parliament in 1889 and 1892 respectively. The right of Scotland to so large a sum is not questioned, since, no doubt, when these Acts were passed the Scots were willing to forgo other privileges in order to make better provision for their own higher education. Our point, however, is this: whatever may be the genesis of the grant or grants, the total sum is relatively much larger than that assigned to England. If such a sum is necessary for Scotland—and we do not doubt it is—surely the Government should see that a proportionate sum should be given to England.

One other point. The Estimates provide for a sum of 500,000*l.* for superannuation purposes. This is intended to be a special non-recurrent grant in aid of certain Universities, colleges, medical schools, etc., to assist them to provide retrospective benefits for senior members of the staffs under the Federated Superannuation System of the Universities. In a previous issue we have already criticised the proposal and expressed the opinion that this sum will fall far short of the amount necessary for the purposes indicated. Unless a grave injustice is done to the senior members of the staffs, the grant will have greatly to be increased, or an opportunity given them to come under the School Teachers (Superannuation) Act. It is certain that a very large number of University teachers would gladly avail themselves of the latter alternative.

Colloidal Theory.

An Introduction to Theoretical and Applied Colloid Chemistry: "The World of Neglected Dimensions." By Dr. Wolfgang Ostwald. Authorised translation from the German by Prof. Martin H. Fischer. Pp. xv+232. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1917.) 11*s.* 6*d.* net.

The Chemistry of Colloids. Part 1, *Kolloidchemie.* By Prof. Richard Zsigmondy. Translated by Prof. Ellwood B. Spear. Part 2, *Industrial Colloidal Chemistry.* By Prof. Ellwood B. Spear. Pp. vii+288. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1917.) 13*s.* 6*d.* net.

AFTER reading the books the titles of which stand at the head of this article, one is inclined to ask whether the word "colloid" as it has come to be used does refer to a definable state of matter, or whether it is not, in fact, used as a convenient label for a heterogeneous group of states which have only this in common, that they are not easily assimilated to the ordinary doctrines of molecular physics.

It is agreed that the word refers to systems in which one state of matter is dispersed through another, but it is claimed that there are no natural boundaries between such systems and coarse settling suspensions on one hand, and true molecular solutions on the other.

Having convinced themselves that there are no natural limits, both Dr. Ostwald and Prof. Zsigmondy select arbitrarily certain sizes of particles or degrees of dispersion and define mixtures which lie between as colloidal. This is a mere confession of weakness, and every step of the argument on which it is based seems open to challenge. It is pure topography, and as such of little value. The colloidal state does, in fact, touch solution on one hand, and suspensions on the other, but it is not a matter simply, or even primarily, of scale. The distinctive quality of the state consists in certain constraints which may fairly be called frictional constraints, from which comes the characteristic inertia of colloidal systems noticed by Graham. An ideal suspension in which the relation between particles and fluid medium was one of simple repulsion would be free from such constraints. Its sole characteristic would be a uniform distribution of the particles—this follows from considerations of entropy—so that, if appropriate external restraints operated, the system would manifest an osmotic pressure.

An ideal suspension of this kind is the ideal gas of colloids, and the distinction between it and the simplest colloidal solution lies in the fact that the particles react with the fluid, the energy associated with the reaction being of the type known as surface energy, but modified by the excessive curvature of the surfaces. Each particle acts as a strain centre, the molecules about it being orientated more or less with respect to its centre, and the total effect is an increase in the rigidity and a decrease in the mobility of the fluid—a decrease that is in the number of molecules which cross unit area of a plane surface in the interior in unit time. Any constraint which the particles exert on the molecules of the fluid will therefore tend to increase their own diffusive energy, and the osmotic pressure would be greater than that of an ideal suspension, just as when true solution is exothermic the osmotic pressure is greater than that given by the gas equation.

The energy peculiar to such systems may be classified as capillary and electrical, namely, a contact potential difference between the particles and the medium. We are ignorant of the quantitative relations between the two, but stability is least when the contact potential difference vanishes—that is to say, at the isoelectric point. This feature is almost always and quite wrongly described by saying that coagulation occurs at the isoelectric point. Coagulation, of course, occurs over a range which is determined by the magnitude of the forces operating to produce agglutination and precipitation.

It is obvious that two particles which come within range of each other will or will not agglutinate according as the variation of surface energy with the distance between their centres is positive or negative. If it be negative there will be a buffer action similar to that which may often be observed between drops of one fluid floating on the surface of another. A finite amount of work must be done to bring about agglutination, and this is an instance of one of the frictional constraints characteristic of colloids. Third components are practically always present in minute amount in actual sols condensed on to the particles. They decrease the chances of agglutination because they decrease the energy of the interface between particle and fluid, and, therefore, help to make the variation of the energy with the distance between centres negative.

We may note in passing that the diffusive energy is concerned only with the distribution of the particles. The size of the particles—that is to say, whether they do or do not agglutinate or completely fuse on "contact"—is determined by the variation of energy mentioned above. A striking example is offered by the system ether–water. If the ether phase be distributed through the water by shaking, the drops are brought into contact again by the external aggregating force gravity, and, once in contact, they immediately fuse. If, however, a trace of iodine be added, gravity brings the drops together; but they do not fuse, because of the local influences of the iodine upon the local variation of energy on "contact."

Having got so far, it does not need much imagination to see that the reason why colloidal particles do not fuse must be essentially the same as the reason why solid faces do not weld when pressed together.

There would be little difficulty in defining the colloidal state if the relations between the components were only those mentioned above. It is at the other end of the scale where sols shade into true solutions in a perplexing way, not because of variation in the size of the particles, but because true solution exists side by side with true colloidal dispersion.

Broadly, there are two types to consider: those in which true solution involves, or seems to involve, the entire colloidal component—*e.g.* silica—and some proteins in water; and those in which the solute is a salt, one ion of which is highly insoluble, in which case the dispersed phase consists of aggregates of this ion with unionised molecules. Such systems are salts of proteins and of fatty acids in water, and the remarkable feature is that though the "colloidal" ion may grow to such a size as almost to reach the limits of microscopic vision, the electric charge it carries is the area of its surface multiplied by a constant.

To return now to the delimitation of the colloidal state. It should be such as to include at one extreme bacteria growing in a medium. It has been shown quantitatively that agglutination of bacteria occurs when the contact potential difference at the surface of the bacteria is destroyed. They, therefore, present a characteristic feature. At the other extreme there would be such a system as turned up accidentally during the war at a certain factory. An oil was found to form with water a stable emulsion remarkable for the size of the drops, which averaged nearly 2 millimetres in diameter. When the drops were broken up by violent shaking they slowly grew to the characteristic large size, and at constant temperatures persisted for months, forming a system defined by a distinct curvature of the interfaces fixed probably by frictional constraints. Clearly delimitation can neither be simply dimensional nor is it to be found in the chemical make-up; it must be sought and is to be found in the presence of characteristic constraints.

Both books present in a fair way the contemporary views of colloidal theory. It is to the theory that criticism is directed, not to their presentation of it. Each book has its peculiar merits. Prof. Zsigmondy, for instance, is particularly good and complete in all that refers to the ultramicroscopy of colloids.

Dr. Ostwald's book gives the substance of lectures delivered in America at the invitation of certain universities. It is a good introduction to the elements of the subject. One special feature may be noticed. The book was completed before the war, and the first preface is dated 1914. Publication was deferred for obvious reasons, and the second preface, dated 1915, was written whilst the author was actually at the front. From that agony of unrest the author sends a message of peace, as dignified as it is just, to his colleagues in what were then enemy countries. For that message of goodwill I for one thank him.

W. B. Hardy.

The Epistemological Problem.

(1) *A Study in Realism.* By Prof. J. Laird. Pp. xii+228. (Cambridge: At the University Press, 1920.) 14*s.* net.

(2) *Studies in Contemporary Metaphysics.* By Prof. R. F. A. Hoernlé. Pp. ix+314. (London: Kegan Paul, Trench, Trubner, and Co., Ltd., 1920.) 16*s.* net.

(1) "Il faut bien plus de principes que vous ne pensez pour démontrer ce dont personne ne doute," observes Malebranche in his "Entretiens Métaphysiques." This came to mind in reading the quotation from his adversary Arnauld which Prof. Laird has placed at the head of the introduction to his "Study in Realism." There can be no knowledge without object known, is the gist of Arnauld's remark. How undeniable! And yet Prof. Laird has to write a book and hint to us that he finds it difficult to keep his study within reasonable bounds. The realists are all alike; they disarm their adversaries by the *naïveté* of their definition, only to discover that there is no end to the diversity of meanings their professedly obvious affirmation may cover. "If the shade of Reid could visit these regions to-day it would greet Mr. Prichard, of Oxford; but it would be startled by Mr. Alexander, bewildered by Mr. Russell, and distressed by Mr. Holt. Indeed, one is tempted to think that any realism defined to the quick becomes nothing but the definer's private philosophy." Such is one realist's confession.

This troublesome problem of knowledge, however, is one to which searchers for truth, whatever be the scientific direction of their inquiry, cannot be indifferent. It is impossible to avoid its challenge, although it is not one of the great problems of philosophy. It is not, like the immortality of the soul, the nature of the world, and the existence of God, one of the problems which concern the whence, the why, and the whither of human existence. The epistemological problem is in effect the River Styx of the higher world of philosophy, but there is no Charon who can be bribed with a fee to ferry us to the other side.

Why is realism called a theory? It is not a theory in any proper meaning of the term. It is simply an assumption concerning the reality of things and the knowing relation, and the contention that the assumption is consistent with the facts. The assumption is that the object of knowledge is independent of the knowing, and that knowledge is discovery, the independent things or objects being directly revealed or given to the mind. This is the ordinary assumption of common sense, but neither the plain man nor the scientific researcher calls it a theory or requires a theory. It is the philosopher who wants a theory. The argument of the realist seems to be that if the assumption can be proved to be consistent with the facts of perception, memory, imagination, and such like processes, it will then become a theory. To this the reply is, "Can the Ethiopian change his skin?"

Prof. Laird is delightful to read. However difficult and abstruse the argument, it is bright with